An Experimental Study of an Unusual Type of Reaction in a Dog.

By G. VAN T. HAMILTON, M.D.
(McLean Hospital, Waverley, Mass.)

With Two Figures.

[Reprinted from THE JOURNAL OF COMPARATIVE NEUROLOGY AND PSYCHOLOGY, Vol. XVII, No. 4, 1907.]

151.3
H18e

Reprinted from *The Journal of Comparative Neurology and Psychology*, Vol. XVII, No. 4, 1907.

AN EXPERIMENTAL STUDY OF AN UNUSUAL TYPE OF REACTION IN A DOG.

BY

G. VAN T. HAMILTON, M.D.
(*McLean Hospital, Waverley, Mass.*)

WITH TWO FIGURES.

These experiments were undertaken in the interests of a problem suggested by such instances of animal behavior as are with difficulty, or not at all, interpretable in terms of instinct or of associative memory, and which may be ascribed to accident or not, according to the sympathies and viewpoint of the observer.

The practical difficulties in the way of limiting, regulating and repeating the stimuli that seem to produce unusual adjustments render the always objectionable apparatus necessary to a sufficiently critical investigation of such a problem; but negative conclusions drawn from results obtained by apparatus experiments cannot properly include a denial of the possibility that such animals as habitually solve their problems by the "trial and error" method of reaction (where instinct does not serve) may, under exceptional circumstances, display a type of reaction which stands higher in the scale of modifiability of behavior. Simplicity of an experimental situation calls for like simplicity of reaction to it on the part of the animal; and any artificial complication of an already artificial situation is apt not to be in line with his general reactive tendencies.

It is to be regretted that, owing to the requirements of my method, I was unable to use more than one animal throughout the 600 experiments; but the efficiency of this method, and the suggestiveness of the results obtained by its use will justify, I hope, the report that is to follow. The subject, a bull terrier of mixed breed, was about four months old when he came into my possession. At that time he knew no tricks whatsoever, had never been trained, and was not used to people and houses. Since then he has had no tuition except what has been necessary to overcome certain objectionable habits. Although he has done the usual number of things that are ascribed to "reason" by the uncritically sympathetic, his everyday life has afforded no behavior that instinct, associative memory or accident will not explain.

Early in his career he was familiarized with a simple apparatus which was equipped with four suspended blocks of wood, one of which, if clawed, would release a door which led to food. These blocks were placed in various positions in the cage, alternately attached to the door-release, and labeled in some manner. Thus, colored cards were scattered about the cage, and a white one was always placed on the block that was attached to the door-release. This was merely for the purpose of making experimental situations a part of his daily life; and especially, to insure that later they would not excite fear or aversion. To a playful, well fed puppy all this meant a good time, food, objects to be chewed up, and the presence of his master after an all day absence, whether he succeeded or not in getting out of the cage. If he did succeed many added and unusual pleasures awaited him, and no suggestion of penalty for failure entered into the game.

This preparatory work extended over several months, and in the end afforded me a subject so well fitted for the formal experiments that the reaction-value of the experimental situations was far greater, I think, than is usually obtainable by ordinary experimental methods.

Description of apparatus.—(1) A wooden frame, 4 feet wide, 5 feet long, 2¼ feet high was covered with coarse wire netting, as was also the lid. (See Fig. 1.) (2) A door (*A*), 1 foot high, 26 inches wide, fitted with a spring to pull it open when the button (*B*) which held it closed was turned. (3) Four wooden pedals (*D*) which were passed through slots in the rear baseboard, and hinged to a floor railing 11 inches to the rear of this. These pedals were held in a slanting position (anterior ends directed upwards) by means of wire springs. Very slight pressure upon their anterior ends was sufficient to move them toward the floor. (4) Four strings, each of which was attached to a pedal at the point of its emergence from the cage; from there the individual string was carried upward to the series of horizontal rings that carried all four strings to a trigger (*C*) at the left side of the cage. The trigger itself, when sprung, released a spring, which in turn pulled the button aside, thus releasing the door.

By hooking a given string to the trigger the pedal from which it came was thus attached to the door-release. (5) Four "pedal cards" (*F*), which were simply heavy wooden boards, 4 inches wide by 11 inches high; they were furnished with two legs each, which fitted into slanting holes in the pedals in such a manner as to give them an upright position when in place. Each pedal card had a different colored paper pasted upon its anterior surface, viz: black, red, green and yellow. (6) Four pairs of odor cards (*G*); these were small wooden paddles with cotton tacked to their anterior surfaces. Each of these was kept saturated with a drug having a distinctive odor, viz: asafœtida, lupulin, castor oil and beef extract. (7) Four wooden sign boards (*E*), 11 inches wide, 3 feet long. Their anterior surfaces were covered with papers matching in color those of the cards. (8) One white sign board and pedal card, and three plain pedal cards.

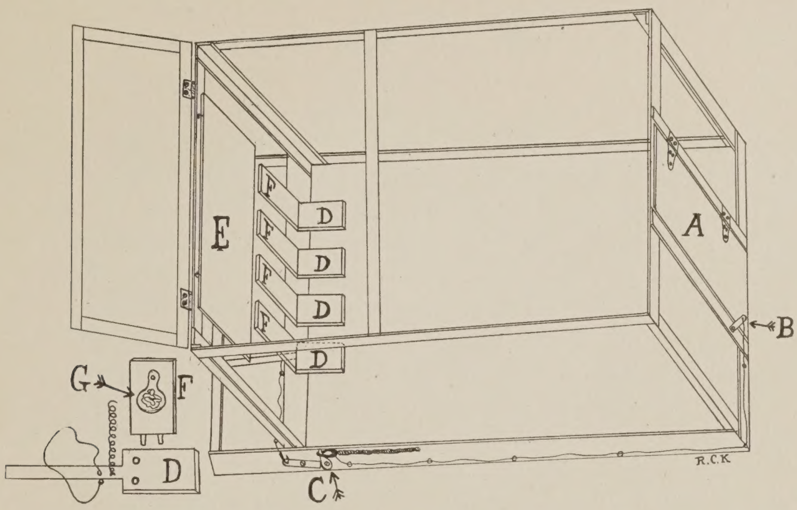

Fig. 1. Diagram of the experiment cage; *A*, spring door; *B*, button with trigger string attached; *C*, trigger with one pedal string attached; *DDDD*, pedals, one of which is shown separately with string and suspension spring; *E*, sign board; *FFFF*, pedal cards, one of which is shown separately, with legs to fit into the holes in the pedals, and with odor card (*G*) suspended from its anterior surface.

Experiments with the white sign board and white pedal card.—In this series of experiments the different pedals were attached, one at a time, and in varying order, to the trigger, the attached pedal always bearing the white pedal card, and the unattached

McLean Hosp 7

16 Mr14

pedals bearing plain pedal cards. The white sign board was kept suspended above the row of pedals throughout this series of experiments. The animal was given 160 trials, 20 a day. In calculating results an error was recorded for each attempt to escape by striking an unattached pedal. If the animal struck the attached pedal first during a given trial a "correct first choice" was recorded for that trial (see table). Time measurements proved to be of absoultely no value, since the animal sought the pedals as his means of escape from the start, and was so dexterous in passing from one pedal to another that there was no essential difference between the time of escape when there were no errors, and when there were several. Besides, as there was no pressing desire to escape, conditions external to the interests of our problem determined the length of his stay in the cage. He always came out directly the shock of the discharged trigger was felt, but often he wandered about the cage contentedly before attempting to escape. As a rule he was fed before experiments were begun; the only incentives for escape which the experimenter supplied were a few much gnawed bones and a commending caress. It has been my experience that a well fed, contented dog, if undistracted by other sense-impressions than those of the experimental situation, is apt to inspect his immediate environment quite thoroughly, and to act with more appearance of caution than has been reported by previous experimenters with these animals When put into the cage for his first trial the dog went directly to the attached pedal, struck it, and came out. Of course his previous experience easily accounts for his having struck at the first projecting object that came his way, and I do not doubt that his immediate success in getting at the attached pedal was accidental. When he was put in for the second trial he went at once to the pedal that had just let him out and struck it, then started for the door; the white pedal card was now on another pedal, and that pedal alone was attached. But the animal did not seem to take the directing pedal card into account, for he returned to the unattached pedal again and again, striking it more forcibly each time, and always running to the door after each attempt. After ten such attempts he sat down by the door and began to whine. He was spoken to reassuringly, after which he returned to the pedals and inspected them. After some hesitation he struck the attached pedal, thus releasing himself. His behavior during the remainder of the 160 trials showed nothing of particular interest beyond a progressive decrease of errors, and an increase of correct first choices. The experiments were discontinued after sufficient data had been obtained for purposes of comparison with those to be obtained from the more complicated experiments about to be described.

Experiments with colors, colors and odors, and odors alone.—At the 161st experiment the white sign board and white pedal card were discarded. The green sign board was hung in position, the green pedal card was placed in p 1 (the first pedal to the left), the black pedal card in p 2, the yellow pedal card in p 3, and the red pedal card in p 4. There were sixteen different combinations of this sort, so arranged that each color was represented upon each "pedal attached" once in sixteen times, and upon each "pedal unattached" three times in every sixteen. Each of the four pedals was attached four times in a series of sixteen experiments. An irregular order of attaching pedals and placing pedal cards was followed in order to prevent the animal from learning a sequence; but care was taken that no one pedal should be attached twice in succession, that no one color should occur as the sign of "pedal attached" twice in succession, and that every pedal card should be shifted to a different pedal after each experiment.

At the 481st experiment the colors were reinforced by odors: red by lupulin, black by asafœtida, green by castor oil, and yellow by beef extract. Thus, if the yellow pedal card (now reinforced by the beef extract odor card, suspended from its anterior surface) were on the attached pedal, the yellow sign board was hung out with a beef extract odor card attached to it. At the 581st experiment the colors were abandoned, and only odor cards and odor sign boards were used. These were attached to plain sign boards and pedal cards, identical in form with those that bore the colors.

It can be seen from the above that an adequate reaction to the situation required the animal first to seek the sign board, then to inspect it, and finally, to strike the pedal bearing the only card that would afford him the same odor or visual stimuli that he got from the sign board. In this connection it may be said that it was impossible to determine positively whether or not he was ever guided by the colors as such, or even by their differences in light intensity, although the latter increased rapidly in the order from black to yellow. It is probable that the colored papers had each a distinctive odor, since the animal not only looked at them, but sniffed them carefully before reacting. It is to be remembered that so long as distinctive stimuli were afforded, and so long as the sign boards gave the same stimuli as their appropriate pedal cards, the principle remains the same. Color discrimination as such is not a part of our problem, and colors were used only for the sake of a possible reinforcing value.

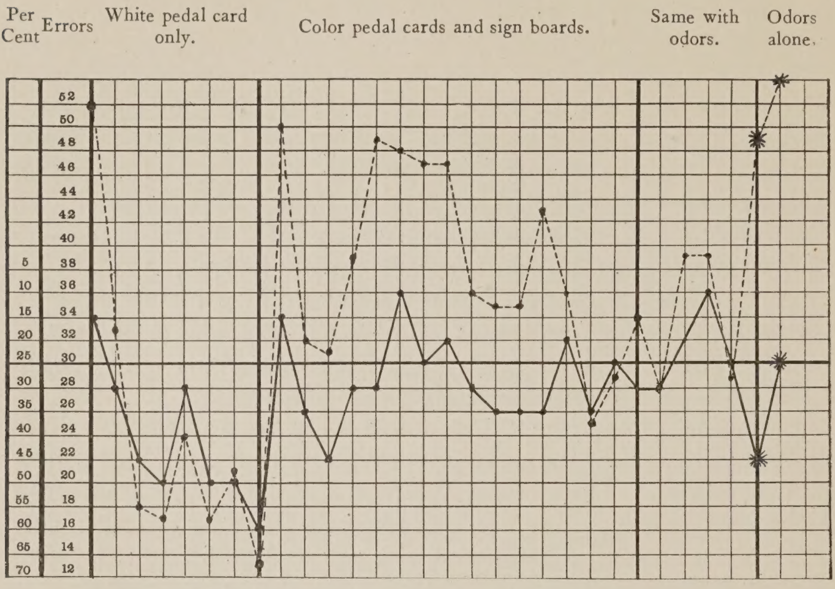

Fig. 2. *Explanation of Table of Curves.* Each of the dots connected by the closed lines represents the percentage of correct first choices in 20 trials; each of the dots connected by the broken lines represents the total number of errors in 20 trials.

Where stars (**) are used in place of dots, sets of experiments in which electricity was used are indicated.

The heavily shaded horizontal line upon which the figures "25" and "30" stand, is the average chance ine mentioned in the text.

Our results demand two general analyses: (1) An analysis of errors, *i. e.*, of the number of times that unattached pedals were struck, and of the factors determinative of these inadequate reactions; (2) an analysis of the correct and incorrect first choices. It may be well to remind the reader that the pedal which the animal struck first during a given trial is recorded as his first choice for that trial.

Analysis of errors.—This scarcely need be given in detail, since a large number of errors during a given trial seemed to be due variously to, (1) mere playfulness and inattention, (2) a persistent effort to escape by returning (during the same trial) many times to the pedal or pedal card that had proved successful at the immediately preceding trial, or (3), when electricity was finally introduced, to fear and a wild desire to escape.

Analysis of correct and incorrect first choices of pedal.—There was such a varying degree of attention to the sign boards and pedal cards, and the formation of so many misleading associations occurred, that a large number of factors determinative of the first choice of pedal is disclosed by the analysis of results. While it seems an almost hopeless task to attempt a complete and adequate classification of these factors, the following will serve, I think, to throw some light upon our problem.

(1) As factor "1" I wish to refer to several closely related factors which can be conveniently grouped together. After the animal had been successful with a certain pedal he was apt to return to it and strike it first for many successive trials. At times this seemed to become a kind of habit with him; he would go to his favorite pedal at once on entering the cage, strike it, and then, failing to get out, inspect the apparatus in a very leisurely manner, and make his second choice.

A preference for p 3 and p 4 was undoubtedly due to the fact that he is "right handed." In clawing things from a table, and in all other acts that call for the use of a single forepaw he almost invariably uses the right forepaw. This is due neither to tuition nor to any injury or discoverable deformity, and has been amply demonstrated by appropriate experiments.[1]

The following table, supplemented by the analysis that follows, will indicate to what extent the complex factor "1" accounts for the dog's first choices:

First choice of the same pedal for 2 successive trials.........	p 1 = 7	p 2 = 11	p 3 = 18	p 4 = 11
First choice of the same pedal for 3 successive trials.........	p 1 = 1	p 2 = 7	p 3 = 11	p 4 = 7
First choice of the same pedal for 4 successive trials.........		p 2 = 2	p 3 = 2	p 4 = 1
First choice of the same pedal for 5 successive trials.........	p 1 = 1	p 2 = 1	p 3 = 1	p 4 = 2
First choice of the same pedal for 6 successive trials.........			p 3 = 1	
First choice of the same pedal for 7 successive trials.........	p 1 = 1	p 2 = 1		
First choice of the same pedal for 13 successive trials.........				p 4 = 1
First choice of the same pedal for 16 successive trials.........				p 4 = 1

---

[1] See BALDWIN, Mental Development, Methods and Processes. Second edition, p. 67. 1903. Also ERNST WEBER, Ursachen und Folgen der Rechtshändigkeit. pp 10–13. 1905. It is possible that my dog is exceptional in this respect.

Obviously, factor "1" does not account for all the 266 first choices given in this table. From this total we must subtract (*a*) the 88 first choices that started the 88 series; (*b*) the 49 correct first choices that occurred in the course of the series (although it is a fair inference that factor "1" accounts for many of this latter group, such an inference cannot be drawn with certainty, since the animal often terminated a series by choosing the attached pedal, only to return to the favorite pedal of the series thus interrupted at the next trial); (*c*) the 42 first choices that occurred as the second member of a series when the initial first choice of that series was successful (such reactions are tabulated under factor "2"); and (*d*) the 25 times when the animal might have been following up a successful color or odor in making his second successive first choice of a given pedal.

To summarize: (1) there are left 266 – 88 – 49 – 42 – 25 = 62 first choices that cannot be accounted for on the assumption of a transiently formed association between the desired result and an immediately preceding successful pedal, color, or odor, or to the influence of the directing sign board. These 62 first choices—all of them incorrect—may be ascribed to factor "1."

(2) In 101 trials the animal made (incorrect) first choice of the pedal that had just let him out.

(3) In 102 trials he made (incorrect) first choice of the pedal bearing the color or odor of the immediately preceding attached pedal. This occurred in 83 (25.9 per cent) of the 320 experiments with colors alone; in 15 (15 per cent) of the 100 experiments with colors reinforced by odors; and in 4 (20 per cent) of the 20 experiments with odors alone.

(4) Out of the total 440 experiments in which colors, colors and odors, and odors alone were used, he made correct first choice of the attached pedal 122 times (27.7 per cent). It is here that we must look for any light that the experiments may have thrown upon our problem. The question at issue is, "Did the presence of the directing sign board increase the adequacy of the animal's reactions to the situations in which it entered?" Inferences drawn from a mere numerical tabulation of results must, of course, be qualified by the character of these 122 objectively correct first choices. A great deal depends on whether his choice of the attached pedal appeared to be haphazard, "hit-or-miss," or to be attended by such preliminary activities as would have made for

greater adequacy of reaction. But the relatively slight influence of the experimenter's personal equation on tabulations of results, and its inevitably distorting influence on any report of the animal's apparent intention, must, so far as one kind of certainty is concerned, give precedence to mathematically derived inferences.

First of all, then, we must take into account the fact that had the animal been uninfluenced by any associations, mere chance would have enabled him to make correct first choices in approximately 25 per cent of the total number of trials. Or, had he always struck the same pedal first, or the pedal bearing the same color or odor, he would have made 25 per cent correct first choices. Now, the 53 incorrect first choices unaccounted for by any of the above-mentioned factors, and the 62 incorrect first choices ascribed to factor "1" (the latter, since they often came in series of more than four each) can most conservatively be considered to be due to nothing that impaired the animal's chances to make the average chance 25 per cent correct first choices.

The case is quite different with the 101 incorrect first choices ascribed to factor "2," where the animal chose the pedal that had just released him; of necessity any choice due to the influence of such an association would be an incorrect one, thus materially lowering his chance of attaining the average 25 per cent correct first choices. But mere chance would have enabled him to strike first the pedal that was last attached in 110 of the trials. This would be a fatal objection to any attempt to demonstrate that the animal was misled by such associations were it not for the fact that the choose-first-the-last-successful-pedal reactions came, not at irregular intervals, but in definite and prolonged series. Thus, if p 3 had just released him, he would strike it on reëntering the cage, and p 1 proving to be the successful pedal, the next time he would make an unsuccessful attempt to escape by striking p 1, and so on, until some new determinative of first choice became effective. The same is true of the 102 incorrect first choices ascribed to factor "3."

It now becomes clear, I think, that to have made 122 correct first choices the animal must have been influenced by the sign boards; otherwise, the effects of the misleading experience-determinants to first choice would have been reflected in a considerably lower than the average chance per cent (25 per cent) of correct first choices.

Passing now to a consideration of the character of the adequate reactions, it should be said that so far as observation of the individual trials was concerned, the majority of correct first choices might have been due to mere chance. But I was able to record 23 incidents like the following. The animal inspected the sign board, pedal cards, and pedals, turned his head to look at me, wagging his tail (my position was at a point 20 feet directly in front of the door), and then, confining his attention exclusively to the sign board, devoted some time to sniffing its surface. Following this he went up and down the row of pedals, sniffing their cards; then pausing at an unattached pedal, he raised his paw as if to strike, but desisted, withdrawing the uplifted paw slowly, without having struck the pedal. Again he returned to the sign board and sniffed its surface, following which he passed down the line of pedals, sniffing their cards as he went, until he came to the attached pedal (*i. e.*, the one affording him the same stimuli as the sign board); this he struck and was released. There was not a single failure when he behaved in this manner.

An examination of the correct first choice curve will show that there was but little, if any, improvement at the end of the 440 experiments with sign boards. This agrees with the fact that the kind of behavior just described never became a fixed mode of reaction. Usually, after he had once reacted so adequately, he quickly chose the previously successful color or odor, thus falling into error.

It is interesting that the odor reinforcement was of so little value to him. That particular series of experiments would have been continued much longer had it not been for the fact that the animal finally learned a very effective and easy manner of finding the attached pedal. Starting at the third or fourth pedal to the right, he would pass to the left, striking the pedals in succession until the shock of the discharged trigger sent him scurrying to the door. If he started at p 3, and p 4 was attached, he would go down the line to p 1 in the usual manner, then walk directly to p 4 and strike it. Reference to the curves shows that in the third from the last set of 20 experiments he made 29 errors, and struck the attached pedal first in 5 of the 20 trials. That he made only 29 instead of the "average chance" 30 errors, is due to the fact that in switching from the method of starting at p 4 (a method which he pursued for 13 successive trials), to the method of starting at p 3, he avoided making one error. It was to break up this habit that electricity was introduced as a penalty for errors. It seemed to be effective during the first 20 trials of its use, but after that all previously acquired fondness for the experiments was replaced by fear, and I found myself working with an animal that reacted like THORNDIKE'S hungry cats and dogs.[2]

LLOYD MORGAN defines a psychological process as "the middle term between results of complex stimuli from the environment on the one hand, and the results of complex reactions to that environment on the other hand."[3] Instead of stating our problems in the interests of hypothetical interrelations of these "middle term" processes, and instead of making our experimental and clinical observations subservient to problems so stated, it seems desirable to pursue a method of studying animal behavior which will keep us more closely in contact with the facts accessible to us. Such a method is realized, I believe, in the clinical[4] and experimental study of reaction-types.

It is true that partially objective methods have been followed in this field so far as the higher vertebrates are conerned, but the divorcement from middle term speculative demands has been more apparent than real. Otherwise, there would be no appeal to "criteria of conciousness;"[5] no catering to hypothetical modes of mental elaboration of sense-data; and, in short, no need of psychological inferences, in our interpretations of animal behavior.

Animal behavior affords data for the solution of a great and comprehensive problem: Starting with the assumption that from the lowest forms of life to human life, there is an ever increasing adequacy of adjustment to complex environments, and that the adequacy (in the sense of complexity) of adjustment implies a corresponding complexity of effective inner elaboration intervening between reception of stimuli and reaction to them, the general problem becomes far simpler, and loses none of its importance if it have to deal only with the possibility of establishing the presence or absence of a continuity of the third terms of the formulæ, stimuli—inner elaboration—reaction. Attempts to conduct experiments for the sake of gaining knowledge of the intimate workings of the "inner elaboration" seem only to retard and unnecessarily to complicate the problem. If the complexity of a given situation be definitely known, and if there be only one "most adequate" reaction possible to that situation, these, and not inferred psychic processes will enable us to give the reaction its continuity-position. Of course, other objectively determined factors must enter into consideration, such as the relative influence of instinctive equipment and of experience.

---

[2] THORNDIKE, E. L., Animal Intelligence. *Psychological Review Monograph supplement*, vol. 2, no. 4, pp. 30–32. 1898.

[3] LLOYD MORGAN. Comparative and Genetic Psychology. *Psychological Review*, vol. 12, p. 79. 1905.

[4] For the lack of a better term "clinical" is used here to indicate the kind of observation that is employed by the psychiatrist in his studies of insane patients. It is generally recognized by clinical psychiatrists that the academically trained psychologist (if he lack adequate clinical knowledge of insanity) is greatly hampered in his experimentation with patients by his lack of clinical checks upon his work. The case is quite analogous where experimental work with animals is not supplemented by prolonged and extensive observation of the subjects dealt with.

[5] See YERKES, Animal Psychology and the Criteria of the Psychic. *Journal of Philosophy, Psychology and Scientific Methods*, vol. 2, pp. 141–150, 1905, for a viewpoint which recognizes the value of psychological inferences.

Returning to an interpretation of the results obtained from the experiments just reported, it is required of us, then, to isolate such reaction-types as appeared there. Of these, two stand out more or less distinctly. (1) During the first 160 experiments, when only the white pedal card was used, the animal showed an increasing adequacy of reaction. Here, amongst several constant factors, the most important experience-determinant (the white pedal card), was the only one that made one situation different from another; but in spite of its changing pedal relations, the white card, in its direct sense-value to nose and eyes, was a simple, concrete, persistently recurring experience-determinant. Innumerable observations, both in the laboratory and in everyday life, have shown that dogs' experience-determined reactions are usually thus conditioned, and that final unfailing and perfect adequacy of reaction to such factors requires their frequent recurrence in situations in which the animal is placed. (2) In the last 440 experiments, where the directing sign board was used, the important experience-determinant was constant in principle, but not in direct sense-value. A perfectly adequate reaction to the situation required that, as the result of previous experience with a concretely dissimilar situation, the dog be so influenced by the stimuli derived from the sign board as to strike first and only the pedal bearing a card which afforded him the same kinds of stimuli. A given situation was repeated only once in sixteen times, and no simple associations could be of any positive assistance to him in guiding him to the attached pedal; on the contrary, they only served to mislead him.

Although such behavior cannot, if my analysis be correct, fall under the head of accidental happenings, and therefore stands out as an actual reaction-type, it did not tend to increase in frequency. Whether the capacity for occasional adjustments of this degree of complexity is a necessary part of the canine endowment, or merely an individual trait, it strongly suggests the possibility of there being still higher reaction-types higher in the phylogenetic scale.

The reader will be better oriented as to the nature of the views and problems just expressed if a brief reference be made to the fact that they are due, in part, to certain investigations in the field of psychiatry. From the extremely inadequate reaction-types observed in certain psychoses, to the relatively adequate reaction-types of relatively normal individuals, there is unbroken continuity. KRAEPELIN,[6] with his keen insight into abnormal reactions, establishes this continuity by means of transitional clinical pictures, which lead us from the abnormal to the normal. And in spite of his objections to the radical degeneracy doctrines of LOMBROSO and the Italian school of positivists, he makes frequent references to the fact that many of the reactions occurring in the "degenerative psychoses" strongly suggest the reactions of primitive peoples. On the assumption of degenerative reversion in certain psychoses, there at once arises the possibility that the apparent gap between human and animal adequacy of adjustment may be bridged by means of studies of reaction-types of the highest of the animals below man on the one hand, and of studies of reaction-types in savages and the degenerative insane on the other hand.

The writer wishes to acknowledge the obligations under which he has been placed by his colleague, Dr. R. C. KELL, who rendered much valuable assistance in the construction of apparatus, the drawing of figures, and the actual carrying out of experimental work.

KRAEPELIN, Psychiatrie, II Band, siebente Auflage. 1904.

3 0112 061798614